the Boltzmann-Maxwell distribution, they will be so distributed at any subsequent instant. But in the absence of collisions or encounters between the system, this distribution will *not* be unique. There exist other distributions equally well satisfying the condition of permanence.

The case of two or more molecules of gas in the process of encounter can be deduced, as shown in my Report (§ 30), by taking each "system" to represent a pair of molecules, there being at every instant an indefinitely large number of such pairs having different coordinates and momenta. It now seems definitely settled that if the molecules are rigid bodies, the only possible permanent distribution unaffected by encounters is the Boltzmann-Maxwell distribution combined with motions of pure translation and pure rotation of the mass of gas as a whole (Report, § 46).

Many writers have limited their investigations to the case where all the encounters are binary, and I see that Prof. Fitzgerald has emphasised this point in his recent letter. I do not think this is the right place to draw the line. I can see no difficulty in taking account of encounters between three, four, or more molecules, provided that such encounters are sufficiently numerous to have a law of distribution. Where the line must be drawn is indicated in my Report, § 30 (iii.). The molecules which act on one another in any one encounter must form an infinitesimal fraction of the whole mass of gas, or the gas must at any instant be divisible into an infinitely large number of independent molecules and groups of molecules, each molecule or group thus constituting for the time being a free system by itself. When the molecules act on each other at all distances, or it is impossible to divide them into small independent groups, the whole of our theory breaks down.

It is this limitation which, to my mind, precludes our applying the Kinetic Theory in its present state, *not only* to molecules moving about in a continuous medium, such as the ether may be, but *also* to solids and, probably, liquids.

A good deal of confusion has, I fear, arisen with regard to what I have called Maxwell's Law of Partition of Kinetic Energy, *i.e.* the statement that "if the kinetic energy of a system be expressed as a sum of squares, the mean values of these squares are equal," from the fact that the term "*mean value*" may be taken to represent "time average for a single system"; and I fancy this may be the point Prof. Fitzgerald had in view when he asked if the conclusions would not apply to the Earth or a "finite number of particles moving about for an indefinitely long time." It cannot be too strongly emphasised that this interpretation of mean values involves assumptions which have hardly been sufficiently justified in any general class of cases, and which have been repeatedly proved to be invalid in simple cases (Report, §§ 9-12).[1] To give a simple (if not quite analogous) illustration: suppose that on drawing counters out of a bag, we were to obtain on the average an equal number of red, white, and green counters. We should *not* be justified in inferring that if we kept a single counter sufficiently long it would change colour and become in turn red, white, and green.

As an illustration of a case where the Boltzmann-Maxwell Law is inapplicable, consider a sphere of density σ moving in a perfect liquid of density ρ. The kinetic energies of the sphere and liquid are in the ratio of $\sigma : \frac{1}{2}\rho$, as we know from hydrodynamical considerations, and the same is true for any number of spheres whose distances apart are very large compared with their radii. This is not a case where *no* transference of energy takes place between the sphere and liquid, and it is not therefore open to Prof. Fitzgerald's objection to the case I previously cited. Whenever the motion of the solid is varied, energy *is* transferred to or from the liquid.

If a *gaseous* ether will satisfy the requirements of physicists, then the Boltzmann-Maxwell Law is undoubtedly applicable to the ether. If not, the ether falls entirely beyond the scope of our investigation. The Kinetic Theory is obviously a theory framed to account for one class of physical phenomena only, viz. the thermal properties of gases. If any observed phenomena are not deducible from the results of the theory, it is to my mind sufficient to show that this is because the fundamental assumptions of the theory are not satisfied, *i.e.* that the phenomena in question are *not inconsistent* with the Kinetic Theory. G. H. BRYAN.

Peterhouse, Cambridge, January 16.

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[1] See also NATURE, January 10, p. 262.

Boltzmann's Minimum Function.

I SAID in my first letter on this subject that the condition A, on which, or its equivalent, the proof is based, could not apply to the reversed motion. As that assertion has been questioned, may I confirm it thus?

The initial distribution of R, the relative velocity, *i.e.* the number of pairs of spheres for which it has given direction, is arbitrary—condition A is fulfilled. Then, as proved, whatever the initial distribution, after collisions, the distribution of R is uniform, *i.e.* all directions equally probable. Now reverse the velocities. If condition A is fulfilled in the reversed motion, then after the reversed collisions the distribution of R is uniform. It is equally certain that it must be the same as the initial distribution.

If, therefore, condition A is fulfilled in the reverse motion as well as in the direct, that can only be because the distribution of R was uniform to begin with. But that means that H was minimum to begin with, and therefore $\frac{dH}{dt} = 0$ throughout.

Boltzmann's theorem can be applied to both motions only on condition that it has no effect in either.

1 New-square, Lincoln's Inn. S. H. BURBURY.

Electroscopes in Lecture.

I SUPPOSE teachers still use gold-leaf electroscopes for their junior lectures; certainly I have found nothing else so dead-beat, or so readily understood; and by projecting with a lens a shadow of the leaves on a square-foot translucent screen, the movements are perfectly visible to a large audience by daylight. But the one objection to the instrument, when used for explaining the fundamental facts, say, of induction, is that it indicates similarly positive and negative potentials. Yesterday, however, my assistant, Mr. E. E. Robinson, ingeniously stood the metal-cased instrument on a cake of paraffin wax, and electrified its outside negatively. By this process the zero of deflection is changed; the leaves stand apart for zero potential, diverge more for positive, and collapse for negative. A zero shadow-pointer and rough scale may be readily used; and I propose now to mount a projection electroscope in a suitable slightly-charged Leyden jar, whose outer coat can then be treated as the usual earthed terminal of the instrument, whose case is connected to or formed by the inner coat of the jar. The insulated or variable terminal is conveniently arranged as an insulated sphere or other shaped body on the lecture table, not far from the small screen, attached by a long enough thin wire to the leaves—of which it is perhaps best to have only one movable. OLIVER J. LODGE.

January 25.

The Perseid Meteors.

YOUR interesting reference (NATURE, January 24, p. 301) to Dr. Bredichin's investigation of the Perseid shower, induces me to offer one or two remarks on the subject. In the paragraph alluded to, it is stated that the radiant was observed from July 22 to September 12, 1893; but this long duration cannot possibly refer to the same system as that which furnishes the abundant maximum on August 10. It is true there are radiants in Perseus in September, and the succeeding months of October, November, and December supply many others, but, after the end of the third week in August, nothing is seen of the real Perseids. I cannot say exactly on how many nights the display continues active, but it is certainly visible from July 19 to August 18, and the diurnal motion of the radiant is about 1° to the E.N.E.

There are really very few of the true Perseids seen after August 12, for the shower dies rapidly away after the maximum. On August 15, 1893, during a watch of three hours, I saw only one Perseid. On August 16, 1893, during a watch of 4½ hours, I noted only four Perseids. I mention these facts to prove the extreme tenuity of the stream at the middle of August. But there are differences from year to year. An observer watching the heavens for a similar interval on the same dates in succeeding years will get a varied experience. Thus one year cannot be regarded as the criterion of all.

Some years ago I recorded a few swift streak-leaving meteors between August 19 and 24, from a radiant at 78° + 56° on the northern borders of Auriga, and supposed they might be late

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Perseids, but subsequent observations have convinced me that they had no relation with the great August shower. They probably formed the early members of a well-defined radiant of September Aurigids which I found at 76° + 56° in 1879, and at 77° + 57° in 1885.

It appears to me that observations of the Perseids, and of other meteor showers, are often undertaken and discussed while losing sight of a most important circumstance. I refer to the necessity of thorough training on the part of the observer, before he can possibly hope to attain a high degree of precision in recording meteor paths. Many months, if not years, of diligent practice are required to render the observer proficient, and even then there are many students who, being deficient in natural aptitude, will never succeed in the work. It seems to be the fashion at certain observatories to set a number of observers (some of whom have perhaps never registered a meteor path before) watching and recording meteors, and then to investigate their results as though they could be thoroughly depended upon. Such results are, however, practically useless when employed to test any complicated point in meteoric astronomy. It is similar to placing a man, who has never played in a cricket match before, as wicket-keeper to fast bowlers like Mold, Richardson, and Woods, and expect his performance to be creditable! In meteoric astronomy, as in many other spheres of action, skill is only to be acquired by long practice; indeed, it is difficult to single out any other branch of observation where the eye and the judgment have to be so quickly and accurately brought into play to afford the best results.

W. F. DENNING.

Bristol, January 27.

The Artificial Spectrum Top.

IN the interesting letters on the above subject, which have recently appeared in NATURE, there does not seem to have been any reference to the experiments of Helmholtz, as described in his "Handbuch der Physiologischen Optik," 1866, § 23. He describes the facts in minute detail, and illustrates them with numerous diagrams.

One important point not yet referred to, and described in detail by Helmholtz, is that if a disc, marked with black and white sections, be rotated with a certain rapidity, the field appears to be covered with a pattern composed of hexagonal spots; at the part of the field of vision corresponding to the yellow spot, a transverse oval figure is seen. In the centre of this figure is a dark spot surrounded by a black circle.

Each of the hexagonal spots is dark with a lighter spot in the centre, and surrounded by a red thread, which appears to be moving in minute drops. The field seems to be pervaded by a greenish hue, which flows towards the yellow spot.

These experiments, which I have verified on every point, have a very important bearing on the photo-chemistry of the retina and on colour vision.

Hendon, January 26. F. W. EDRIDGE-GREEN.

IN reference to your Belfast correspondents' interesting experiments with the artificial spectrum, which were long ago included in my own experiments, a little reflection will show that when the speed of rotation is increased, we do *not* retain unaltered the resultant proportion of stimulus and anti-stimulus on the retina. With a slow rotation we have simultaneously on the retina a persistence image of the lines and a real image of the white card. When the speed is greater, we get simultaneously these two, and in addition a persistence image of the white card. Hence, according to my theory, the rise in scale with increased rapidity of revolution.

Colchester, January 26. CHARLES E. BENHAM.

Snake Cannibalism.

THE reading of a paragraph and a letter printed in the *Mail* for October 24 and 29, reminds me of a case of one snake swallowing another, the consequences of which I witnessed. While engaged in running a survey line for a railway across a wood in this district, I noticed a snake close to me, doing its best to get out of my way, but almost unable to do so. One of my men struck at its neck with his "macheti," and succeeded in cutting the snake's head clean off. Immediately, to our great surprise, another snake of the same species slowly emerged head first, and, after a few struggles to escape, remained motionless on the ground; a gash in its cranium, which had been cut by the same stroke that killed the larger snake, being, no doubt, the cause of death, as the body was otherwise intact. A measuring tape showed that the larger snake was 6 feet in length, and the smaller 5 feet. In this case the snake was swallowed tail first, and therefore it seems highly probable that the larger snake simply devoured it, and did not commence by trying to dispute a portion of food, such as the pigeon and frog cited in other instances.

H. TSNAGAL.

Sancti Spiritue, Cuba, November 23, 1894.

More about Moths.

(*Communicated by Prof. S. Garman, of the Museum of Comparative Zoology, Cambridge, U.S.A.*)

IN NATURE for December 6, 1894 (p. 127), Mr. Henry Cecil publishes a criticism on a previous letter of mine, which I cannot accept without a few words of remonstrance. His explanation may be correct in part, but it certainly does not cover all the ground.

That resistance alone is not necessary for the expansion of the wings of moths, may be inferred from the fact that they will often expand after an interval of several days, when the moths have been prematurely released, the irregularity in outline arising, I think, from the evaporation of moisture from the wings, and in the consequent loss of elasticity. If the newly-hatched insects are confined in a warm moist box, this trouble seems to be obviated in a large degree, and the wings occasionally attain to nearly the normal dimensions.

In raising moths artificially, it cannot be assumed that the lack of proper pressure is entirely responsible for the frequent occurrence of cripples.

All the conditions of feeding, moisture, and heat, must first be carefully considered, since departures from the normal, on any one of these lines, might so lower the vitality of the insect, that perfect development would become impossible.

The writer also speaks of the wings of the moth in the cocoon as "folded and crumpled," a statement which is entirely at variance with my own observations. In all the cases which I have noticed, the wings are perfectly smooth and unfolded from the first, the increase in size resulting from a true expansion, the nature of which has, so far as I know, never been fully explained.

L. C. JONES.

Melrose, Massachusetts.

THE PHYSICAL SOCIETY'S ABSTRACTS OF PHYSICAL PAPERS FROM FOREIGN SOURCES.

THE days when learning meant dead languages, and science meant collecting beetles, have passed away; science has grown and spread until it is impossible for the most comprehensive intellect to grasp more than a few twigs on its numerous branches. Organised specialisation has become necessary to scientific progress. Each subject now has its special society, and each society has as much as it can do. Every sort of time-saving arrangement is necessary if the workers in any one branch of knowledge are to be kept informed as to what others are doing.

English chemists have long been supplied by the Chemical Society with excellent abstracts of the current literature of their subject, but up to the present the only available work of the same kind on Physics has been the *Beiblätter* of *Wiedemann's Annalen.* Admirable as these are, it is impossible that a German periodical can fully meet the wants of Anglo-Saxon physicists. It is therefore most desirable that abstracts of physical papers should be published in English. The Physical Society has now set itself to supply this want, and the first number of the new volume of "Abstracts" appeared early in the present month. The *Proceedings* of the Society will in future be issued monthly, and the abstracts of foreign papers on physics will be included under the same cover. They will, however, be paged separately, so that they can be bound separately at the end of the

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